

It would thus appear that the readings obtained through the wooden model are sensibly the same as those obtained through the central cube in the ordinary manner, and that the discrepancy is due to a real difference between the lines of collimation of the central and excentric portions of the object-glasses of the Collimators; and the application of a correction to the "concluded reading for collimation," as deduced from an adjustment of the North Collimator through the cube, is accordingly justified.

With regard to the observations before 1874 which were not so corrected, Sir George Airy remarked that for clock stars and other fundamental stars near the equator, the effect of the correction was merely differential, and therefore almost insensible, and even for other stars is a small quantity.

It may be remarked generally that this investigation suggests that the use of excentric pencils of light in observations of this kind, without proper care, is liable to introduce unexpected errors.

On the Protuberances Visible on the Spectrum with a Narrow Slit.
By E. L. Trouvelot.

At the last meeting of the R.A.S., March 12, Mr. Maunder read a paper containing some criticism on a spectroscopic observation of mine, published in the January number of the *Bulletin Astronomique*. After having described the observation Mr. Maunder said: "I think it is sufficient merely to state this observation to show that there must be some mistake somewhere." Further on he continued: "But the most important thing is that he saw the prominence not only when the slit was open but when it was closed," and he concluded by saying that "the observation was evidently impossible."

If I understand correctly the objections, according to Mr. Maunder, the observation was impossible: 1st, because a protuberance having a certain height, say $3' 37''$, cannot be seen with a slit open wide enough to admit it; and, 2nd, because such a prominence cannot be seen whole with a narrow slit. I must confess that these objections, coming as they do from an experienced observer, have caused me quite a surprise, for the first phenomenon is not so difficult to detect even on ordinary occasions; and the second is not so rare that a diligent observer might not see it at least once or twice in the course of a year. That protuberances projecting upon the spectrum have not yet been seen by Mr. Maunder is quite possible, since these forms of prominences are rare and have usually a short duration; but I am at a loss to understand why he should have failed to see protuberances $3'$ high with a wide-open slit, when I can see them so easily, for, I suppose, the instruments he uses for his observations are equal to mine. I will state that in ordinary circumstances—that is, when the sky is passably clear

and the prominences tolerably bright—I make my daily drawings of these objects with a tangential and wide-open slit, permitting the whole form to be seen at a glance. In this way I have often drawn prominences 3' and 4' high, and indeed I have sometimes thus recognised the complicated structure of protuberances having twice this height. Of course the brightness of these objects diminishes with the widening of the slit, and the conditions of seeing vary with the clearness of the sky, the brightness of the prominences, and the aperture and perfection of the instruments employed. But here we only refer to ordinary hydrogen protuberances, which are not very bright; while those seen on the spectrum, which all belong to metallic eruptions, are usually so very brilliant that their structure is very distinct with a wide-open slit.

The projection of large prominences on the spectrum, such as those of Sept. 10, 1872, and of June 26, 1874, are very rare, and they are indeed so much so that these are the only cases which came under my observation in a daily experience of fifteen years with the spectroscope; but the smaller forms of projections, having an elevation from 30'' to 60'', and even 80'', are not so rare, and on an average I have observed them two or three times every year. I am persuaded that if Mr. Maunder be cautious to cover his head with a veil, and allow a few minutes for his eye to get accustomed to darkness, he will see prominences with a wide-open slit as I do; and if he be assiduous and patient, it will not be a very long while before he sees a bright and well-formed protuberance on the spectrum, although the slit of his spectroscope will be narrow and almost closed. I do not know whether Mr. Maunder has founded his objections on any other grounds than those above quoted, and reported in the *English Mechanic* for March 19, p. 51, where I read them. But, supposing he has other reasons, I do not see that it would change anything in the observed phenomenon; the fact will remain unchanged. I think it would be quite surprising if the remarkable phenomenon I have observed for the last thirteen years, and described more than eight years ago, had not been seen by at least a few of the numerous observers looking daily at the Sun. If it has not, it may be expected that it will soon be recognised now that the attention of observers is awakened. As far as can be judged from the brief postscript added to the note of M. Tacchini, “sur la distribution en latitude des phénomènes solaires pendant l'année 1885,” published in the *Comptes Rendus*, March 15, 1886, it would appear that the phenomenon in question has been recently recognised by this observer. The P.S. reads thus:—“Les 9 et 10 Mars courant, j'ai observé au bord ouest du soleil, des éruptions métalliques très-singulières, dont la matière projetée et tout à fait éblouissante rendait comme continue et très-vive une portion du spectre à partir de la raie C vers D, et l'apparence était celle d'une protubérance vue devant le spectre.” On March 9, at 10 A.M., I observed a bright prominence situated at

261°, and following a group of Sun spots which passed on the other side the night before. This object, whose elevation was 2', reversed a great number of metallic lines, and was so intensely brilliant that, with the slit over 2' wide, it could be seen beautifully with all its complicated structure, not only on the hydrogen, magnesium, and helium lines, but also on the red line whose wave length corresponds to 6676·8 (?) It is very likely that it is in the vicinity of this object that the Italian observer saw the eruption above described, but he probably did not observe at the same time as I did, for although of a dazzling brilliancy, no projection on the spectrum except a slight deviation of the C line towards the violet end was seen at the time mentioned.

The following experiment made on the prominence form projected on the spectrum on Sept. 10, 1872, may have some interest, and will perhaps throw some light on the subject. The C line was made tangent to the Sun's border, just as shown on fig. 12 of my last Paper, only the slit was made so narrow that the C line appeared sharp and black. The narrowing of the slit did not prevent the protuberance remaining wholly visible on the spectrum, as I have said before. The telescope being clamped, and the clock in motion, the C line and Sun's limb kept relatively together during the experiment; then, by means of its proper screw, the slit was moved away from the Sun's limb, sliding along the prominence form from its base to its summit. As the slit advanced the lower part of the prominence became invisible, being hidden as it were by the portion of the spectrum situated on the less refrangible side of the C line, which was by no means diaphanous for it, like the portion on the opposite side of the same line. As the slit continued to advance, only the upper portion of the prominence situated on the most refrangible side of C remained visible, and it disappeared when the slit reached its summit. It is to be noticed that although the protuberance was bright on the spectrum, it was entirely invisible on the dark C line, except at one or two places where it was seen crossing this line, and even advancing a little on the spectrum towards the red end. The same phenomenon was also observed on the prominence of June 26, 1874, but then the displacement on the opposite side of the projection was at places quite considerable.

Whatever may be the cause of this singular phenomenon, which as a positive fact will stand, I am not prepared at present to discuss, but it is certain that, as far as my observations go, it agrees well with the theory attributing it to the motion of the source of light in the line of sight.

Note on M. Trouvelot's Paper. By E. W. Maunder.

It is to be regretted that M. Trouvelot did not wait until he had seen the actual text of my paper before replying to it, as the report on which he based his reply was a very inaccurate one. I did not call in question his ability to see the whole of a large prominence at once through a widely opened slit, nor even the possibility of his seeing an entire prominence projected on the spectrum. But I pointed out that one of his observations, taken as it stood, involved, according to the accepted interpretation of the displacement of lines in prominence spectra, that the prominence in question was moving with every conceivable velocity from rest up to 2,584 kilometres per second. We cannot imagine the possibility of such a state of things, of a body being at one and the same time in a state of rest, of gentle motion, and of rapid motion; clearly either the observation must be incorrect, or the interpretation of it.

M. Trouvelot's present paper makes it clear, I think, that it is the observation that is in fault. Taken as it stands it amounts to the assertion that with a slit only a few seconds wide, and placed tangentially close to the Sun's limb, he was able to see an object nearly $2'$ away from the limb, or much more than $1\frac{1}{2}'$ outside his slit. In other words, that on occasion he is able to see a prominence through the solid metal of his slit-plate; or rather through one plate of his slit, for whilst one plate was diaphanous to the prominence the other remained opaque.

It is clear that there is some error in the report of such an observation. Probably, and this would account for the entire phenomenon, the slit, when M. Trouvelot tried to close it, refused to answer its screw, as slits will occasionally, and therefore remained fully open during the entire observation. However this may be, it is clear that any spectrum can only correspond to the light actually admitted through the slit, and that the displacement of lines *within* that spectrum cannot bring within the range of the slit objects that are placed far outside it.

For observations of displacements of the lines of prominences M. Trouvelot would find a narrow radial slit much preferable to a wide tangential one, and if instead of reversing his spectroscope as a check on his observations he had made his slit radial, he would at once have seen whether these marvellous displacements were real or not.